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THE LATE DUDLEY WILMOT BUXTON,
M.D., M.R.C.P. (Lond.).

Consulting Anæsthetist, University College Hospital, Royal Dental Hospital, London, and Hospital for Paralysis and Epilepsy, Queen's Square; Physician Anæsthetist West Herts Hospital: Fellow Royal Society of Medicine and Ex-President of the Society of Anæsthetists; President Section of Anæsthesia, International Medical Congress, 1913, etc.

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In Memoriam

Dudley Wilmot Buxton, M.D. (Lond.), M.R.C.P.

Consulting Anæsthetist, University College Hospital.

We regret to record that Dr. Dudley Buxton died on June 28th last, in a nursing home, on his seventy-sixth birthday. His death removes from the ranks of British anæsthetists one who has for long been a respected leader, and it deprives the British Journal of Anæsthesia of a man who from the first gave it his valuable support, who was always ready with wise advice when it was sought and who not only could, but did, provide admirable matter for its pages whenever his services were requested. Qualifications of the kind possessed by Dr. Dudley Buxton are not commonly discovered, and his will be no easy place to fill. His life was a long one, and as its latter years were passed in comparative retirement his name, no doubt, to our younger generation of anæsthetists, does not carry the full significance that it has for those who learned their profession while Dudley Buxton was an active force in London. The former know him only from his writings, the latter remember, too, the tall, eloquent, and witty teacher and speaker whose personality impressed itself on the minds of those who were aspiring to walk in his footsteps, at however far a distance behind. Had it not been for a natural defect in delivery Dudley Buxton would have been a very fine speaker, but even as it was there was no man more eagerly listened to, or better worth hearing, than Dudley Buxton on any question concerning anæsthetics when it was debated at a medical society during the time when he was at his zenith.

Possessed of a clear brain, a sense of humour, and no mean ironic power Dudley Buxton was listened to with interest by all, though with some apprehension by opponents. The qualities which made him so acceptable a speaker among equals made him also a fine teacher of students, and innumerable men who passed through his hands at University College Hospital and at the Royal Dental Hospital bear his name in grateful memory. In the profession at large he was presumably best known by his textbook "Anæsthetics" which reached its sixth edition in 1920, when it was more than twice its original size. Buxton's last communication to our pages was made as recently as January of last year, when he furnished an article on "The Mentality of the Patient and its Bearing on the Phenomena of Anæsthesia," a side of the subject with which he had recently been increasingly engrossed.

Dudley Wilmot Buxton was the son of Mr. Harry Wilmot Buxton, barrister-at-law of the Middle Temple. He was educated at private schools and at University College, London. Entering University College Hospital in 1878 he took his M.B., and B.S. (London), with double honours in 1882, and after serving as house-surgeon and house-physician took his M.D. in 1883 and the M.R.C.P. in the following year. His leaning at the time was towards general medicine and Buxton was appointed assistant to Sydney Ringer, then Professor of Medicine at University College. Later he decided to devote himself to the specialty of anæsthetics, then in its early days of scientific development, a development in which Buxton himself played a prominent part. He became anæsthetist to University College Hospital as well as to the Royal Dental and the National Hospital, Queen's Square, and from his clinical work and from experimental work which he carried out he acquired an amount of knowledge which he was able to impart, both in his verbal teaching and in numerous papers to the medical publications of the time.

Some of the most valuable work which he did was as Secretary to the Special Chloroform Committee of the British Medical Association and also in connection with the Hyderabad commission on Chloroform, in regard to which we may

quote from the obituary in the *Lancet*. "At this time Buxton was the advisor of the *Lancet* on the subject of Anæsthetics, and his labours in connexion with the Hyderabad Commission, undertaken for us, were of a difficult character. In 1888 the Nizam of Hyderabad instituted an inquiry into the action and effects of chloroform . . . various comments on the conclusions were made. As a result, the *Lancet* was invited to send a representative to hold a further inquiry on the spot. Lander Brunton was selected for the task . . . the results of the inquiry were published as a supplement to the *Lancet* in 1893 under the editorship of Dr. Dudley Buxton who summarised, with extreme care and ability, the voluminous records, and furnished a clinical addendum."

Buxton's work will be long remembered as seeking to place the administration of chloroform on a scientific basis and to limit the dosage accurately, for which purpose he used and frequently recommended the Vernon-Harcourt inhaler. In practice he favoured nitrous oxide and ether much more than chloroform and regarded with favour the increasing use of the former agent for long operations, although the most recent machinery for facilitating that object came, as did endotracheal work, too late for him to employ it with familiarity.

Buxton married in 1884 Louise, daughter of the Rev. Thomas Grey Clarke, and leaves three sons, of whom one is lecturer in physical anthropology at the University of Oxford, one, surgeon at King's College Hospital, and one, Dental surgeon at University College Hospital.

SPINAL ANÆSTHESIA, SPINOCAIN AND DURACAIN.

By E. Falkner Hill, M.B.

Lecturer in Anæsthetics in the University of Manchester.

EARLY this year Professors E. Forgue and A Basset published a book on spinal anæsthesia, the embodiment of twenty years' experience with novocain. The account they give of it forms a suitable standard with which to compare the Pitkin method, or rather the modification of the Pitkin method, which I use. As novocain is the anæsthetic used in both methods this fact facilitates comparison and rules out all discussion of other anæsthetics such as stovaine, percaine, etc.

Technique. The injection is made in the lumbar region in the middle line in any suitable space. I generally use that between the second and third lumbar vertebræ. Professors Forgue and Basset inject 2 c.c. of an 8 per cent solution of novocain and take eight minutes over the injection. They thus use only 80 mg. of novocain. For a high abdominal operation they increase this dose to the maximum of 120 mg. The dose I use ranges between 200–300 mg. according to the height of analgesia required. The injection takes about one minute to perform. It is possible to use this larger dose, thanks to the presence of gliadin, a viscous body which tends to hold the solution together. From this feature flow two results. Firstly, there is little tendency to diffusion of the anæsthetic, and so we very rarely see vomiting due to the spread of the anæsthetic to the bulb, an occurrence so constant with the method used by Professors Forgue and Basset as to be described by them as classical. Thus on page 42 they say, "A cette dose minima, l'agent chimique excite surtout le centre vomissement." Secondly, the presence of this body slows the absorption of

the novocain, so that there is no necessity to take such a long time over the injection as eight minutes, and it spreads the action of the anæsthetic over a much longer period. Thus by diminishing the spread and slowing the absorption we gain more control over the height and are able to give the operator a much longer time; two enormous advantages. It should be pointed out that the presence of a viscous body for the purposes already alluded to, though a characteristic of the Pitkin method, has been utilised before, and was referred to as long ago as 1918 by Rood. How much farther it goes back I do not know. Pitkin used strychnine, but I have long since discarded it as being, in my opinion, undesirable. The reason for this I gave in the paper in the *Lancet*, January 18th, 1930. Fifteen per cent of alcohol is added to make the solution lighter and the necessary quantity of normal saline. Ampoules containing 3.5 c.c. (novocain 10 per cent) are made for me by Messrs. May and Baker of Battersea. Whatever quantity of this solution is required is mixed with more or less cerebro-spinal fluid and injected into the theca. As the dose is all-important and depends on many considerations, the next subject to be discussed is that of the factors upon which the dose depends.

Factors influencing the dose. The two objects that we try to obtain are the correct height and the right length. The margin at our disposal in operations below the umbilicus is wide, and such operations as a rule present little difficulty. It is quite otherwise with operations above the umbilicus, and here the experience of the anæsthetist is severely tested. The following factors must be correctly gauged if a successful result is to be obtained:—

1. The dose.
2. The quantity of cerebro-spinal fluid with which it is mixed.
3. The quantity of cerebro-spinal fluid allowed to escape before injecting.
4. The force with which the injection is made.
5. The inclination of the table.
6. The position of the patient at the time of, and immediately following, the injection.

7. The age of the patient.
8. The build of the patient, including height, weight, and whether thick set or delicately made.
9. The state of the patient's health.
10. The state of the patient's mind.

It is at once obvious that an increase of dose means an increase of height in anæsthesia. An increase in the amount of cerebro-spinal fluid, with which the dose is diluted, also has the effect of raising the height; this is more marked according to the amount of cerebro-spinal fluid that has been allowed to escape before the injection is made. The anæsthesia from a forcible injection will be much higher than that from one made gently. If the table is in the Fowler position a higher anæsthesia may be expected than if it is in the Trendelenburg position. The sitting position gives the highest anæsthesia of all. Pitkin warns us never to use it. Patients over forty, other things being equal, require a slightly smaller dose than younger ones. I should expect to have to give a patient of twenty-five to thirty-five a larger dose than one of forty-five to fifty-five, to get the same satisfactory result. Whether this is due to a different mental outlook or some subtle change in the constitution of the nerve roots and spinal cord I am not in a position to say. The height, weight, and build of the patient are factors to be taken into consideration. A stocky, thick-set man, even though short, will require a larger dose to produce a given height of anæsthesia than a more delicately built one of somewhat greater height. The state of the patient's health has a marked influence on the dose. Thus, Jonesco says: "Les individus débilités par des hémorragies, par les chocs traumatiques ou nerveux, les cachectisés, les infectés, et surtout les cancéreux, ne peuvent supporter qu'une quantité d'anesthésique qui serait incapable de donner l'anesthésie à un patient en bon état général." The state of the patient's mind has on several occasions prevented the appearance of anæsthesia until after this state has been completely changed. Thus a patient who gives such signs of distress at the stroking of a needle across the skin as to prevent any further attempt to begin the operation will, after being put under

$CHCL_3$ for five minutes, stand a laparotomy of an hour's duration without any further anæsthetic.

Seeing, then, that the success of a given injection depends on the correct appraisement of all these factors, control is not absolute: there is some risk; but Professors Forgue and Basset say: "Qu'il diminue au fur et à mesure que l'on se fait la main." This is as true of the Pitkin technique as of theirs.

Preliminary injections. Atropine should always be given. Morphia is undoubtedly a help in very many cases; it soothes and calms the patient, and for this purpose is in a class by itself. I am decidedly of the opinion that it should be given as a rule. But there is one disadvantage. In the event of the development of anoxæmia from any cause, if the respiratory centre is already depressed by morphia the anoxæmic condition may become so profound that the patient by his own unaided effort is unable to rescue himself, seeing the mechanism by which he would do so normally is paralysed. However, it is a great mistake to attempt a high abdominal operation in a nervous subject without either morphia or ether to the stage of unconsciouness. The ideal is to get muscular relaxation with the spinal and unconsciousness with ether: a very small quantity is required.

Results of injections.

1. Analgesia, character of, success of.
2. Fall of blood pressure; fatalities.

Analgesia. Professors Forgue and Basset have noted some irregularity or patchiness in the distribution of the analgesia produced by their technique. They quote with approval the sixth observation of Pitres and Abadie, which says: "Nous avons pu constater, quelquefois en plein térritoire analgésique, un ou plusieurs îlôts plus large que la paume de la main . . . au niveau desquels les piqûres et les brûlures étaient douloureusement perçues." I have never seen any such irregular distribution of analgesia. This may, of course, be due to having missed these areas, but it may also be due to the fact that the viscous material in the injection holds the analgesic solution well together, and so prevents irregularity.

Percentage of successful injections. The operations done under spinal anæsthesia naturally fall into two classes, those above the umbilicus and those below. The latter group calls for little comment; if the operation is finished without resort to a general anæsthetic it is a success, but if not it is a failure. This simple test is not applicable to operations above the umbilicus. In this region the vagus has very extensive ramifications, and no attempt is made to reach the vagal nucleus with the spinal anæsthetic. Consequently impulses arising from manipulation of the abdominal organs supplied by the vagus will reach this centre and give rise to characteristic effects. If the peritoneum on the under surface of the diaphragm is involved in these manipulations then the phrenic will also take its part in the conveyance of afferent impulses. The effects of the stimulation of these two nerves is always unpleasant, frequently unbearably so, especially if the patient has not had a preliminary dose of morphia. How is the situation to be met? There are several alternatives:—

1. Abandon spinal anæsthesia for operations above the umbilicus, except in the case of the Irish and other heroes and in those who are so desperately ill that such discomfort as is produced passes almost unnoticed.
2. Give morphia beforehand.
3. Give sufficient $CHCL_3$, ether or gas and oxygen just to render the patient unconscious.
4. Inject novocain into the neck of the stomach to anæsthetize the vagus.
5. Give a small sub-anæsthetic dose of avertin, paraldehyde, sodium amytal, etc.

To abandon spinal anæsthesia in operations above the umbilicus is to abandon it in the field in which it gives its most striking results. It is here that the complete muscular relaxation which spinal anæsthesia alone gives is most useful, for it does away with the necessity of powerful retraction (a prolific cause of post-operative thrombosis according to some surgeons), makes the use of the bridge exceptional, and saves 30 per cent of the time spent on the operation. Because it is impossible to point to the individual life saved by these striking advantages they are apt to be overlooked,

but who can doubt that in the sum total of operations they materially lessen the mortality? If, then, spinal anæsthesia is to continue to be used for high abdominal operations something must be done to render them less objectionable.

Though there is something to be said against all the proceedings mentioned here, it seems to me that the preliminary injection of morphia and the giving of the necessary amount of some general anæsthetic are decidedly preferable to allowing the patient to suffer the discomfort, often severe, that he otherwise has to put up with.

To compel a patient, very frequently a nervous dyspeptic, to face a high abdominal operation with consciousness undimmed is asking more than most human nature can stand.

I have no experience of the preliminary use of avertin, paraldehyde, etc.; it is a very definite addition to the technique, and sooner or later complications tend to disappear. If in these high abdominal operations the spinal anæsthesia produces compiete relaxation of the abdominal muscles it has done all that can be expected of it. Whether the addition of an amount of a general anæsthetic is required to cause unconsciousness depends on the temperament of the patient rather than on the height of the analgesia. If muscular relaxation of the abdominal muscles be taken as the criterion of success then there were seven failures in the last hundred cases (up to September, 1930). Of these hundred cases there were forty-nine below the umbilicus and fifty-one above. It was in the latter group that all the failures occurred. They were partial failures in that either the analgesia was not high enough or did not last one hour.

The blood pressure. It is universally recognized that the injection of an anæsthetic into the spinal theca is apt to be followed by a fall of blood pressure, and that this fall usually, but by no means always, varies with the height of the anæsthesia. Consequently, some means to counteract this fall or to neutralise its effect is found in the technique of all who practice spinal anæsthesia. Professors Forgue and Basset are so impressed with this action of the anæsthetic that they say: "La source majeure des contra-indications: c'est l'action hypotensive de la rachi anesthésie." They recognize that the fall of blood pressure may be brought

about in two ways, one by paralysis of the V.C. and the other by the height of the injection, which, by reaching the bulbar centres, enfeebles the heart. Thus they say: "Il faut admettre que ces phénomènes hypotensifs sont produits par la hauteur de l'impregnation toxique," and add: "C'est bien alors le moteur cardiaque qui est frappé"; they do not explain how the rising of the anæsthetic to the bulbar centres damages the heart. They conclude: "De là cette règle de sagesse: ne pas rachianesthésier les hypoten des graves, avec basse pression aggravée de l'insuffisance du moteur cardiaque." On the other hand. many experienced spinal anæsthetists are of opinion that spinal anæsthesia is the method of choice in all cardiovascular diseases. Thus Labat says: "The heart having light work to perform takes a rest during spinal anæsthesia, and for this reason it is the method of choice in all cardiovascular diseases." Pitkin, Kelly, Stout, and others have all expressed a like opinion.

Such polar differences on the part of experts can only be resolved by a patient investigation of the facts. And for this most of them are disqualified, as their practice has been so uniformly successful that on their own showing they have never seen a death on the table during spinal anæsthesia. As a contribution to this investigation I propose to give the details of four patients, three of whom died on the table and one did not. If the usually accepted theory that death is due to the height of the anæsthesia acting on the bulbar centres be correct, either by its influence on the respiration or on the heart, the one that did not die should have done so and those that died should have recovered. That is to say, that in the three who died there were factors other than the mere height of anæsthesia which caused death, which certainly in two and almost certainly in the other one was not so high as in the case that recovered. Here are the details:—

Case 1. Female aged 70. Forty-one years' history of gall-bladder trouble. Three cubic centimetres of spinocain were injected into the theca at 9.23 a.m. between the second and third lumbar vertebræ. Just prior to this injection 50 mg. of ephedrine had been injected subcutaneously. The patient was put into the Trendelenburg position 10° and the

operation commenced. At 9.30 a.m. the blood pressure was 60; the night before it had been 130. At 9.40 a.m., the abdomen having been opened and the gall bladder found, the patient, who appeared quite comfortable, asked how many stones there were in it. The surgeon replied to the question, and then asked to have the table put up. I raised it so that the reading on the tiltometer indicated 5° Trendelenburg. At 9.45 a.m. I noticed that the radial pulse had disappeared, and about this time the patient stopped alternately contracting and relaxing her right hand, which up to that time she had been doing. I then put the table back to a somewhat steeper Trendelenburg position, and commenced artificial respiration, as there was no sign of breathing. She never recovered. The points worth noting are:—

1. The patient was well until the time of the moving of the table.

2. The anæsthetic was not as high as the eighth cervical nerve.

3. Death was not due to the height of the anæshetic; it was due to the moving of the table, though the tiltometer registered 5° Trendelenburg. It was afterwards found that the head of the table had been slightly raised, and as there was a pillow on it there is no doubt that the position of the head favoured the depletion of the bulbar centres of blood. No efficient means of artificial respiration was at hand.

Case 2. Male aged 55. Gangrenous gall bladder with spreading peritonitis. Very ill. The usual preliminary morphia gr. $\frac{1}{4}$ and atropine gr. 1/60 and also 50 mg. of ephedrine had been given. Novocain 0.275 grm. with strychnine, gliadin, and alcohol were injected in the sitting-up position between the second and third lumbar vertebræ mixed with 3.25 c.c. of cerebro-spinal fluid, making in all an injection of 6 c.c. The patient lay on the table slightly in the head-up position and was asked to draw up his legs, which he did. Shortly after (seconds) he felt his legs slipping down the slope of the table. He was immediately put in the Trendelenburg position, but seemed distressed and unable to breathe. I should judge that he had a feeling of tightness in the chest. He gradually became cyanosed, and died. While he was in the Trendelenburg position and in

distress he had a radial pulse, quite definite although feeble, rapid and irregular. He died eight minutes after receiving the injection. His blood pressure before the injection was 120 mm. Hg. The following points are worthy of note:—

1. The patient was desperately ill to begin with.

2. We have no knowledge of the height of the anæsthesia, but judging by the rate at which the legs were paralysed and the sequence of events compared with other cases I am of the opinion that the superior level would have been found in the higher dorsal region.

3. It is certain that the distress in the chest was acute and preceded the complete collapse of the radial pulse. Death in this case was apparently due directly to the anæsthetic, as it occurred within eight minutes and the operation had not started. How did the anæsthetic cause death? Let us assume that the anæsthetic reached no higher than the third dorsal. In this case it would have caused probably, but not certainly, some fall in the blood pressure. This fall in the blood pressure would normally make the heart's work easier. "The heart having light work to perform takes a rest during spinal anæsthesia" (Labat). But there is another possibility. A patient whose blood pressure was only just sufficient to propel the blood through narrowed coronary arteries might be the subject of acute cardiac failure if that blood pressure were somewhat suddenly reduced, as it is liable to be in high spinal anæsthesia. The distress of which this patient complained resembled that of coronary thrombosis rather than that of angina, seeing that it came on while the patient was at rest and with a falling rather than a rising blood pressure. Or, again, it may have been due simply to the inability of the heart muscle to get sufficient blood.

The acute distress of which the patient complained may possibly have had its origin in acute failure of the R.C. Thus J. S. Haldane, speaking of acute respiratory failure, says: "The patient may feel that he cannot expand his chest to breathe, just as if it were mechanically constricted; and he rapidly develops asphyxial symptoms . . . these attacks are very dangerous and many sudden deaths are due

to them." These cases are promptly relieved by oxygen; but this one wasn't. Also there is no reason to suppose that the anæsthetic was anywhere near the R.C., and there is some evidence to the effect that, even if it had been near, the R.C. would not have been paralysed thereby. I think, therefore, that this patient died because the anæsthetic lowered the blood pressure and the heart itself was in such a condition that it could not stand such a fall as occurred. There is definite evidence that the fall was not extreme. There are not sufficient data to prove anything, as no post-mortem was obtainable. I am of the opinion that the evidence is decidedly against paralysis or even fatigue of the R.C. and in favour of heart failure owing to the fall of blood pressure, which in this particular instance the heart could not afford.

Case 3. Female aged 41. Disease of gall bladder. Short and very fat; fifteen stones, blood pressure 100. The usual morphia gr. $\frac{1}{4}$ and atropine gr. 1/60 were given one hour before, and as the blood pressure was low I gave 100 mg. of ephedrine instead of the usual 50. Three cubic centimetres of spinocain were mixed with 3 c.c. of cerebro-spinal fluid and injected in the lumbar region. As the patient was so fat it was impossible to identify any of the lumbar spines, so I could not tell which space received the needle.

The injection was made at 1.4 and the operation began at 1.11, the position of the table being Trendelenburg 10°, the blood pressure 130. The appendix was removed. While this was being done the patient's face had a definite bluish tinge, an oxygen tube was put in the mouth and the colour subsequently improved. There was some slight discomfort on exploring the œsophageal opening, following which the colour was not quite so good, but the pulse was of good volume, regular and normal in rate. At 1.20 the bridge was put up, the blood pressure just (one minute) previous to this being 130. The pulse, as usual, was not quite so good and the colour went somewhat. The operation continued and the gall bladder was removed. Though the pulse was still palpable it was definitely worse, and so I put the bridge down. The heart failed at this moment and no

cardiac pulsation was ever again felt. Time, 1.30. No treatment was of any avail and, moreover, never looked like being so. The following points are worthy of note:—

1. The time of death was twenty-six minutes after the spinal injection.
2. The blood pressure immediately before the bridge was raised was 130 mm. Hg.
3. The gradual deterioration of the pulse subsequent to the raising of the bridge.
4. The suddenness of the final heart failure.
5. The bluish face on assumption of the Trendelenburg position, relieved by O_2.
6. The obesity of the patient.
7. The extreme rapidity of the operation under these very adverse circumstances.

There can be little doubt that death was due to heart failure, but unlike case two it was not due to fall of blood pressure but due to the bridge preventing an adequate return of the venous blood to the right heart—a well-known cause of heart failure. The blueness of the face is evidence of anoxæmia induced by the Trendelenburg position and relieved by O_2. In this connexion it would be well to consider the effect of the Trendelenburg position. J. S. Haldane says: "We found the recumbent posture greatly favours the development of periodic breathing and therefore of anoxæmia." This he ascribed to the increased resistance thrown on the diaphragm by the liver and other abdominal organs which, at any rate, no longer assist the descent of the diaphragm as they do in the upright position. He goes on to say, "It is evident that in the recumbent position the tendency to irregular distribution of fresh air in the lung-alveoli with any given depth of breathing is much increased, so that anoxæmia from this cause is much more readily produced." If the recumbent position is responsible for all this, much greater will be the effect of the Trendelenburg position. In the particular case that we are here concerned with, all these factors were at their maximum. The weight of the abdominal walls added to the weight of the contents must have produced a quite unusual degree of

anoxæmia which the preliminary dose of morphia would accentuate rather than relieve. The patient survived all this, and indeed was in good condition to the moment of putting up the bridge.

The post-mortem examination revealed a greatly dilated left ventricle, in which there was no corresponding hypertrophy; also the pituitary gland was found to be diseased (tuberculosis).

Case 4. Male aged 59. This patient was jaundiced, and had been so for some months. He had refused operation hitherto, but became so much worse during the night that he had consented. The usual morphia gr. $\frac{1}{4}$ and atropine gr. 1/60 were given, and as the operation was expected to be a high abdominal section I gave him 3 c.c. (300 m.) of duracaine between the first and second lumbar vertebræ. The table was 5° head-up position. The action of the anæsthetic was so rapid that it cut a sentence in half which he had framed in answer to a question that the surgeon put to him. The Trendelenburg position was immediately adopted. Three minutes after he had received the injection the pulse entirely disappeared. He was still breathing. Two unsuccessful attempts were made to give him an intravenous injection. Fifty minutes after the injection the blood pressure was just recordable at 40 mm. Hg. The operation began five minutes later.

Several feet of gangrenous gut were removed. The patient went back to bed in another thirty-five minutes still unconscious. He was still breathing and his blood pressure had risen slightly to 45 mm. Hg. He recovered consciousness during the night and lived another six days. Surely if a high spinal injection will kill, this man ought to have died on the table. He was desperately ill; he had far too big a dose of novocain considering his condition, which was even more desperate than had been expected, and his heart had suffered the toxæmia of jaundice for some months.

Consideration of these four cases and others forces me to conclude:—

1. That a high spinal anæsthesia (novocain-gliadin) will not kill even a grossly toxic heart or paralyse the R.C. in the concentration here given up to 300 mg.

2. That patients whose V.C.s are paralysed are in a precarious condition and must be put in a slight Trendelenburg position and kept there. Movement of any kind is strongly to be deprecated. Steep Trendelenburg is not without danger.

3. That the most dangerous cases are those whose hearts are incapable of working at a lower pressure than their normal.

4. With the exception of this condition a fall of blood pressure, however great, provided the patient is not removed from the Trendelenburg position, is quite compatible with safety. I have seen two cases in which the fall was over 140 mm. Hg., and both made excellent recoveries, and are still alive.

5. That a high spinal anæsthesia by lowering the blood pressure puts the patient into a condition which, though perfectly safe in itself, may, by certain intra-abdominal manipulations, such as severe traction on the stomach, evisceration, etc., rapidly pass into a condition of shock. The change is so quick that it would appear to be due to trauma conveyed by the vagus rather than by toxin.

It is usually contended that spinal anæsthesia protects the patient from shock. Thus Tyrrell Gray and L. Parsons, in their Arris and Gale lectures on the "Mechanism and Treatment of Shock" in 1912, say: "We entered on this research on account of the extraordinary control of shock observed in children with the use of spinal anæsthesia." Apart from psychical shock, trauma and toxins (histamine) are recognized as the two great causes of shock. It is improbable that spinal anæsthesia can do anything to lessen shock due to toxins, though it will not add to it as ether does. Gray and Parsons showed quite convincingly that spinal anæsthesia prevented shock due to trauma. From clinical observations I am definitely of opinion that shock is the cause of some of the fatalities that occur under spinal anæsthesia, and is frequently the cause of the alarming condition which patients undergoing a severe laparotomy present. If spinal anæsthesia prevents shock when the operation is confined to the region anæsthetized (and this seems to be beyond doubt), this constitutes no reason for

supposing it will prevent shock when the area of operation is supplied by nerves which are unaffected by the spinal anæsthetic; this is the condition of affairs in a high laparotomy. The vagus, which has extensive ramifications in the viscera down to the region of the cæcum, is entirely unaffected by the ordinary high spinal anæsthetic. The consequence is that the patient is shocked. Let us suppose that the patient is anæsthetic to the nipple line. The blood pressure has fallen 20–30 mm. Hg. The incision is made and is painless. The peritoneum is opened and the examining hand introduced into the abdominal cavity. Some expression of discomfort on the part of the patient follows. When the manipulations are severe the blood pressure falls rapidly. The pulse, which before was full, becomes thin, and in certain cases disappears altogether. The thin pulse is due to intense vaso-constriction together with the dilatation of the capillaries and loss of fluid from them. Owing to this loss of fluid from the circulation the heart fails, partly on account of not having enough blood to contract down upon and partly because its own blood supply by the coronary arteries is insufficient for its own nourishment. Under these circumstances, if an injection of vasopressin is given and an intravenous injection of saline, the pulse becomes full and the blood pressure rises from forty or below to its original height in ten minutes. The vasopressin contracts the capillaries and the saline restores the necessary bulk of fluid to the circulation.

At what particular point of the procedure it is advisable to apply these restorative measures must be left to the combined judgment of surgeon and anæsthetist. The most convenient time is at the end of the operation; it would probably be safer to do it immediately the blood pressure goes down to 40 mm. Hg.

These measures would almost certainly have saved Case 1; it is unlikely that they would have made any difference to Case 2 or to Case 3, whose heart failure appeared to be due directly to the raising of the bridge.

Advantages of the Pitkin Method.

1. The presence of a viscous material which holds the

fluid together and so prevents irregular distribution of it enables us to give a larger dose than is the case if this ingredient be omitted. The larger dose gives the operator a longer time.

2. There is a conspicuous absence of vomiting, probably due to the same reason, namely, the presence of the viscous material.

3. Patchiness is unknown.

4. The greater control makes the use of spinal anæsthesia in the upper abdominal region safer and more certain, whereas Professors Forgue and Basset say: "Pour les régions hautes, les échecs sont fréquent et il semble bien que là on a demandé souvent à la R.A. plus qu'elle ne peut donner" In this series there have been only seven failures, all of which were rendered adequate by a very small amount of general anæsthetic.

Indications for spinal anæsthesia. There is probably no subject upon which experts differ more than on this question, thus:—

1. Jonesco, than whom there is no greater authority in Europe, says: "I am sure, after a practice of nearly twenty years, during which spinal anæsthesia has been the method I have used exclusively in all my operations, from the head to the feet, that it is an admirable method provided that the technique, simple and easy, at which I have arrived after so many years of practice and study, is used."

2. Labat, on the other hand, says: "They (speaking of this and other similar methods of high spinal anæsthesia) still involve an element of risk, much greater than when spinal anæsthesia is intended for operations below the diaphragm." He goes on to say that for these latter spinal anæsthesia is absolutely safe when all the details of technique are scrupulously observed (Trendelenburg). Accordingly, Labat would use it in all cardiovascular conditions, because the blood pressure is lowered and the heart's work made easy, but not in perforations or peritonitis or strangulated hernia, because of the fact that as the vagus is not paralysed it is free to over-exercise its function of motor nerve to the intestinal tract, undeterred by the sympathetics which are paralysed.

3. Stout and other American authors are convinced that some of the best results obtainable with spinal anæsthesia are just in these cases of perforations and strangulated hernia.

4. Gray and Parsons consider, as a result of their investigations into the phenomena of shock, that spinal anæsthesia is strongly indicated in children.

5. Most people consider diabetes and other toxic states a strong indication.

6. Old people generaly take it very well and those who are very ill.

7. A strong contra-indication to a general anæsthetic, such as the presence of lung trouble, constitutes an indication for spinal anæsthesia.

8. In all cases where muscular relaxation would be likely to make the operation easier spinal anæsthesia is strongly indicated.

9. Professors Forgue and Basset consider the region below the umbilicus to be the true domain of spinal anæsthesia, especially emphasizing its value in gynæcological and other pelvic surgery. They say: "Mais c'est surtout pour le chirurgie abdominale (sous ombilicale) et pour les interventions pelviennes, qu'apparait la superiorité de la rachi-novocainisation sur l'anesthésie generale." They do not, however, regard the region between the umbilicus and the diaphragm as beyond the proper bounds of spinal anæsthesia. Indeed, they recommend it for gastrectomy and for some gall bladder cases. In these latter they feel that each case should be judged on its own merits, and for the worst of them give this piece of advice: "Il vaut mieux alors anesthèsier ces malades a l'ether . . . le risque est moindre et la responsabilité moins immédiate."

Contra-indications to Spinal Anæsthesia.

1. There are certain contra-indications to spinal anæsthesia about which there is no difference of opinion, for example, diseases of the meninges, cerebellar tumours, epilepsy, syphilis, etc.

2. But here the unanimity ends, and opinions range from those who would do every operation from the head to

the feet (Jonesco and others), to those who would restrict its use merely to those cases that stand no chance of survival under a general anæsthetic. By a strange illogicality some use spinal anæsthesia only for those grave risks, whilst refusing it as too dangerous where the risk is much less.

3. The most striking difference of opinion, perhaps, is evident in cases of cardiovascular diseases, many regarding these cases as peculiarly suitable for spinal anæsthesia owing to the lowering of the blood pressure which normally occurs, giving the heart, for the time being, an easier task. On the other hand, Professors Forgue and Basset regard a low blood pressure as a strong contra-indication, especially if it is accompanied by "l'insuffisance du moteur cardiaque." With the introduction of ephedrine a low blood pressure in itself is no longer a contra-indication to spinal anæsthesia, but a feeble heart, from whatever cause, adds much to the gravity of the situation, which under these circumstances needs the most delicate handling.

4. Unless there is complete co-operation between surgeon and anæsthetist and efficient means at hand to do immediate artificial respiration if required, spinal anæsthesia above the umbilicus is contra-indicated.

What is the nature of the precarious condition into which a spinal anæsthetic up to the second dorsal puts a patient? In the course of evolution man has assumed the upright position. He has been enabled to do this by developing a V.C. mechanism, by means of which he can control the calibre of his abdominal blood vessels, and hence his blood pressure, in the upright position. When he is given a spinal anæsthetic this mechanism is put out of action for the time being. Certain disabilities follow. The most obvious of which is that, if he be put in the head-up position he will bleed into his abdominal vessels and die from failure of circulation through the brain. This was the mechanism of death in the first case. But there are other disabilities. Haldane has shown that the recumbent posture favours the development of anoxæmia as stated above. Anoxæmia will rapidly lead to asphyxia, the normal response to which is a rise of blood pressure, but under the circumstances no rise of blood pressure can take place. The necessary adaptation

cannot be made. The R.C. remains unsatisfied and fails to supply the heart with sufficiently oxygenated blood. The rapidity of the heart failure is inversely as its strength. That means with a weak toxic or degenerated myocardium death takes place very quickly.

What is to be done in case of accident? Here again there seems to be great diversity of opinion. The following proceedures have been recommended:—

1. Artificial respiration.

2. Intravenous or intracardiac injection of adrenalin. Better still, vasopressin and saline.

3. Lumbar puncture and withdrawal of cerebro-spinal fluid plus the intrathecal injection of caffeine.

Whilst many are in favour of artificial respiration others condemn it as useless on the ground that no air can be made to enter the lungs owing to the intense contraction of the bronchioles, as in an asthmatic attack. Spinal puncture and the injection of caffeine is objected to on the ground that it necessitates moving the patient and is likely to interfere with the asepsis of the operation, and prevents artificial respiration, which Professors Forgue and Basset consider of fundamental importance.

Animal experiments recently performed at the University strongly support artificial respiration as the means above all others which will rescue any animal, however low its blood pressure; and, even after the exhibition of ten times the dose of novocain necessary to induce complete paralysis of the R.C., artificial respiration, supplying the heart with oxygenated blood, maintains life until the novocain has all been absorbed and the R.C. is once more free. It is essential that the artificial respiration be efficient and immediately applied. Exact details of these experiments will be published shortly.

Conclusions. Spinal analgesia for a high abdominal operation in a patient of suitable temperament or so ill that temperament is no longer a factor is, if the analgesia is the right height, ideal. Spinal analgesia, unshielded by morphia, for a high abdominal operation in a patient of unsuitable temperament, a victim of chronic dyspepsia, is a torture to the patient and the anæsthetist. Such patients should be made

unconscious before the operation begins. The surgeon then has the abdominal muscles relaxed by the spinal anæsthetic and the patient is unconscious with one-sixth of the anæsthetic required for muscular relaxation. There remains the question of the right height. As this depends on so many factors already enumerated it is bound to be of the nature of an ideal rather than an accomplished fact; "Il y a un tour de main à acquérir." "Les échecs, incidents et accidents de la R.A. se raréfient d'autant plus qu'on se familiarise davantage avec sa technique." In operations below the umbilicus failures very seldom occur.

Modifications of the Pitkin Technique.

Duracain is better than spinocain in that it contains no strychnine, a body which diminishes the internal resistance of the spinal cord to the passage of afferent impulses. As the whole object of this type of anæsthesia is to prevent the passage of afferent impulses from the periphery to the centre and from the afferent side of the reflex arc to the efferent, it would appear to definitely contra-indicate the use of strychnine. In actual practice it is not unusual to find that the abdominal muscles are becoming rigid before pain returns when using spinocain.

I use a 10 c.c. syringe with an eccentric exit, sometimes mixing the cerebro-spinal fluid with the duracain before injecting, sometimes allowing the cerebro-spinal fluid to sink to the bottom of the syringe and injecting without mixing. Barbotage is not used. The opinions expressed are based on an experience of between 500 and 600 cases, of which I have blood-pressure records of 472. Of these 472 recorded cases, 245 were for operations above the umbilicus and 227 were for operations below.

PRESIDENTIAL ADDRESS.

Read before the Section of Anæsthesia, Victorian Branch of the British Medical Association, Melbourne, January 15th, 1931.

By H. W. Hornabrook, M.B., M.R.C.S.

(Abstracted.)

PRIOR to 1908 the status of the anæsthetist in Victoria or, indeed, in any of the States of the Commonwealth was not all it might have been.

There was only one honorary anæsthetist on the full staff of the Melbourne Hospital in those days—the late Dr. Embley—and when a request was made by a few of us to strengthen our standing on the honorary staff it was met with considerable opposition from some of the honorary staff. Our persistency, however, had its reward in the end.

After many weary months of hard work I was called upon to place the position before a full meeting of the Melbourne Hospital Board.

Over twenty years ago the anæsthetist had practically no standing. He was simply the individual who administered the anæsthetic. He had no voice as to what anæsthetic he should administer, and the surgeon considered that he knew far more about anæsthetics and their administration than any anæsthetist.

The fight for recognition on the part of the anæsthetist was very strenuous and led, at times, to wordy warfare.

The outcome of the meeting of the Melbourne Hospital Board was that four anæsthetists were made full members of the honorary staff with the rank of Senior Anæsthetists. They were Doctors Embley, Yule, Trinca, and myself.

Having tackled the Melbourne Hospital it became comparatively easy sailing, and soon we had two honorary anæs-

thetists as full members of the Women's Hospital Staff, one as full member of the Eye and Ear Staff, and one on the Dental Hospital Staff.

Having secured improved status on the various hospitals our next move was to take steps to form a Society of Anæsthetists. This was early in 1909. However, at that time there were only two men, Dr. Embley and myself, devoting virtually our whole time to anæsthetic work, and the others were generally looking upon their anæsthetic appointments as stepping stones to other appointments—so nothing came of it.

However, we did not let matters rest. At the various Medical Congresses papers on Anæsthesia were read from time to time in conjunction with the surgical section (there was no section of our own).

In March, 1913, I had the honour to read a paper before the Victorian branch of the British Medical Association entitled, "A Plea for the More Considerate Treatment of Patient and Anæsthetist by the Surgeon," in which the suggestion was again thrown out that a Society of Anæsthetists should be formed in Victoria (*Australian Medical Journal*, April 26th, 1913). Attention was drawn to the fact that no special section had been set aside for anæsthetists and their work at the Congress, to be held in Auckland early in 1914. The question was taken up, but it was then, however, too late to reach finality. It was referred to at the Auckland Congress and decided that if possible a section should be formed for the next Congress.

However, the Great War broke out towards the end of 1914, and many of those vitally interested were scattered abroad, some never to return. The seeds had, however, been sown; others took on the work, and last Congress in Sydney saw the fulfilment of those early desires, to be followed later by the formation of an anæsthetic section in connection with the Victorian branch of the British Medical Association—the first of the Australian States to form such a branch—and I sincerely hope that the success met with here will encourage others to follow.

I do wish that the modern anæsthetist would get a psychological knowledge of his patients, and endeavour to handle

them less as cases to be anæsthetized and more as human beings with feelings that are very susceptible at the time.

The handling of a patient prior to and during induction is almost more important than the actual prolonged administration.

If I were asked, "What are the main faults in anæsthetic administration to-day?" I would not have the least hesitation in saying:—

Firstly, *Rush Inductions*. This is a common and almost universal fault. A properly administered "push" induction under any form of general inhalation anæsthesia is one of the most pleasant experiences one can have. It is a most delightful drift into oblivion. It can, however, be made a "perfect hell" by rush methods and over-concentration during the very early stages. The only general anæsthetic to which this remark of "delightful drift into oblivion" does not apply is a straight-out ether induction; but even in the case of ether the sensations need not be unbearable if men will induce quietly with plenty of air and with the head and shoulders well raised. Personally I never give a straight-out ether induction, but use either open ethyl chloride (and this also should be used sparingly at the outset, and not rushed as so many men do) or else a few drops of chloroform with the mask, which is of one layer of flannel only, held well away from the face and gradually brought near, and as the soothing effects of the chloroform vapour act upon the larynx ether is added to the mask, so that the actual induction is really with ether. Always during induction the head and shoulders are kept *well raised* at a natural angle. This raised position of the head and shoulders during induction is a most important point. All light should be shut off from the patient's eyes, not only during induction, but also throughout the operation from start to finish. It is really quite unnecessary either to look at or touch an eye at any time during induction or administration. The cutting off of light completely from the eye tends to a quieter and more even induction. It also permits the continued administration of an anæsthetic under a lighter stage of anæsthesia for operative work, and is thus of advantage to the patient in that less anæsthetic is consumed, or, in other words, there

is less poisoning of the patient by the anæsthetic, and so a more favourable convalescence is promoted with freedom from post-operative vomiting and without any risk of post-operative broncho-pneumonia due to the over-doping of the patient. The least an anæsthetist can do is to learn the art of giving his patient "the pleasure" of a decent induction. This can never be attained by rush methods.

Now as to the second big fault of the average anæsthetist, a fault by no means confined to the novice. Many experts with years of experience also fail in this respect:—That is, "over-doping the patient with general anæsthetic." To see some men at work, even men with big reputations, one would think that they were endeavouring to find out how much anæsthetic they could pour into the patient without killing him, instead of endeavouring to maintain a surgical anæsthesia with the absolute minimum consumption of anæsthetic.

There is not one operation, from the crown of the head to the sole of the foot, that cannot be performed with advantage in a stage of anæsthetic sleep with just deepening up during certain stages when nerve areas are being touched up or organs dragged about. To maintain a patient at the same depth of anæsthesia throughout an operation is the stamp of the incompetent anæsthetist, not of the competent one. It is for this reason that an anæsthetist should not only know his anatomy and the nerve areas being handled at the operation, but he should also have a rough knowledge as to what the surgeon is doing or attempting to do.

Ether still stands out as the anæsthetic of choice above all others, if men will learn to handle it correctly. The average patient, if properly handled, can be kept fully surgically anæsthetized with from three to four ounces an hour after he has been properly induced. It is no wonder that anæsthetists who use from twelve to sixteen ounces of ether upon their patients in an hour find them liable to post-operative vomiting and a tendency to broncho-pneumonia. Not only do these men render their patients liable to the foregoing post-operative complications, but during the operation they will get abdominal rigidity more or less marked, due to their failure to supply the patient with a sufficiency of air; for in a majority of cases, when a patient is surgically

anæsthetized, it is the asphyxial condition, brought about by this over-concentration of the anæsthetic, which is the main factor in causing muscular rigidity. It is air, not anæsthetic, the patient wants. The anæsthetic that gives you surgical anæsthesia with a minimum of anæsthetic and a maximum of air is the one that gives you the best relaxation. So we see chloroform and ether standing out in this respect above the more modern inhalation anæsthetics in use, namely, nitrous oxide, ethylene, somnoform, and ethyl chloride.

The analgesia periods are also more marked and prolonged under chloroform and ether than they are under the more modern inhalation anæsthetics. In fact, the analgesic periods, both prior to full surgical anæsthesia and especially following upon surgical anæsthesia, are far more easy to attain and maintain under chloroform and ether adminstration than they are under the more volatile fluid anæsthetics, somnoform and ethyl chloride, or more markedly, the gaseous anæsthetics, nitrous oxide and ethylene. With the gaseous anæsthetics, ethylene and nitrous oxide, there is far too rapid a transition from the conscious through the subconscious into the unconscious stage to be able to get a good analgesic period and to maintain it; and inversely, in coming out of the surgical anæsthetic stage, with both these anæsthetics there is far too rapid a recovery for good analgesia.

With the fluid but highly volatile anæsthetics, somnoform and ethyl chloride, it is easier to maintain an analgesic period, and this analgesic period is more prolonged when coming out of the anæsthetic than in the case of either ethylene or nitrous oxide. With the less volatile and heavier specific gravity fluid anæsthetics, ether and chloroform, the analgesic period is far more prolonged than with any of the other anæsthetics. This is of considerable advantage in that there are very few operations indeed that cannot be carried out, either in part or throughout, in this analgesic stage with advantage to both surgeon and patient, especially so if a preliminary small dose of 1/8th gr. morphia and 1/150th gr. atropine has been administered beforehand, and the patient's eyes well covered so that no light can reach them during the course of the operation. In fact, it is quite possible to maintain a stage of surgical anæsthesia, even for abdominal

operations, with quiet regular respirations and excellent muscular relaxation, and not to administer a drop of anæsthetic, either chloroform or ether, for the last three-quarters of an hour, or last half-hour, of the operation, and for the patient to remain in perfect condition for operative work, and yet, on removal of the eye bandages, open his eyes and even answer questions and then go off into a quite sleep with good, regular pulse and respirations on being returned to bed.

The anæsthetic of the future—one might almost say of the present—will undoubtedly be an anæsthetic sleep in which the *controlling* influence will be an inhalation form of administration, by which means the absolute control will not be entirely removed from the hands of the anæsthetist at the time of operation.

As regards this anæsthetic sleep, there are also grave objections to it, if this sleep is to be prolonged for some lengthy period after the completion of the operation, with slow, shallow breathing tending towards stasis in the lungs and an increased risk of post-operative broncho-pneumonia.

We are seeing to-day a tendency for many anæsthetists and surgeons to indulge freely in the use of such drugs as amytal, luminal, avertin, paraldehyde, etc. Papers are written by enthusiasts as to the great advantage of these drugs in not only lessening the quantity of inhalation anæsthetic required during an operation, but also in the great soothing effect they have on the patient, the quietness of the respirations, and the *prolonged* period of quiescence and freedom from pain that they give on the patient's return to bed. Now, this prolonged period of quiescence and respiratory shallowness is one of the very worst features of these drugs in that this diminution of respiratory volume tends to bring on congestion of the lungs and post-operative broncho-pneumonia. Avertin should never, nor is it intended by its makers that it should, be used at any time as an anæsthetic *per se*. It permits very small quantities of ether to be administered to obtain a full surgical anæsthesia, and thus conduces to better relaxation and quiet respiration. The use of avertin as an anæsthetic *per se*, in the same way as the use of any of the other drugs as an anæsthetic *per se*,

will sooner or later land the surgeon or anæsthetist who so uses them in the death of his patient, for which he (the surgeon or anæsthetist) will be morally responsible. In other words, it is manslaughter: and because the drugs were administered with the best intention in the world it does not excuse the fact.

I have yet to come across the drugs, or combination of drugs, that will surpass the small dose of morphia and atropine given about one hour before operation. This small dose of morphia and atropine, as a routine, should not exceed 1/8th gr. morphia and 1/150th gr. or 1/120th gr. atropine, unless the anæsthetist is looking for trouble sooner or later in some form of respiratory depression. I have been using the preliminary dose of 1/8th gr. morphia and 1/150th gr. atropine, prior to operation, for practically all my cases above 12 years of age for the past twenty-two years, and have never once come across a case of respiratory depression from it; but I have known several cases of respiratory depression, and even some fatalities have been reported to me, following upon the injection of from 1/6th gr. to 1/4th gr. of morphia in conjunction with the administration of a general inhalation anæsthetic. However, we cannot blame the morphia if the man who administers it misuses it by giving an excessive dose. It is not the morphia's fault. This small dose of 1/8th gr. is just sufficient to calm the patient and to relieve his feeling of apprehension prior to induction. It also assists to lessen the quantity of anæsthetic required to maintain surgical anæsthesia. It does not predispose towards post-operative vomiting as it is claimed to do by some objectionists to the method who have given larger doses than the 1/8th gr. There is no question that in the larger doses you may meet with an occasional patient in whom morphia has a tendency to bring on nausea, and it is not necessary to give that patient an anæsthetic to demonstrate the fact.

In the same way a 1/4th gr. of morphia may bring on respiratory depression and shallowness of breathing without any anæsthetic being administered. To say that the 1/8th gr. morphia is so small a dose, that given prior to the adminstra-

tion of a general anæsthetic it really does not count one way or the other, is absolutely and entirely incorrect. It does count.

During the past two or three years a series of experiments have been carried out upon myself at the Research Laboratory in conjunction with the Melbourne Hospital, and also at the Women's Hospital, Melbourne, under the various anæsthetics, chloroform, ether, nitrous oxide, ethylene, somnoform, and ethyl chloride. There was a distinct benefit shown from the preliminary injection of 1/8th gr. morphia and 1/150th gr. atropine. In spite of the smallness of the dose, and always provided it was given from three-quarters of an hour to one hour before induction with general anæsthetic, I always felt a nice soothing, comforting feeling, with a slight, but pleasant, creepy feeling extending up the calves of the legs, and with the eyes well bandaged against all light, and reclining comfortably in an easy chair, or lying on the operating table with the head and shoulders well raised, drifted off into anæsthetic sleep peacefully and quietly.

The only occasion on which I showed any marked excitement was towards the end of some nitrous oxide experiments—I had previously had two administrations of nitrous oxide, one reclining in an easy chair and the second when lying on the operating table. This second experiment had been followed immediately by respiration being obstructed by the nasal mask being closed and the mouth closed. My blood pressure was raised from 144 m.m. to 160 m.m. in a quarter of a minute, and 164 m.m. in half a minute. This was at once followed by my third administration of nitrous oxide, and it was during this induction, which was given lying on the operating table, that I struggled slightly, moved my arms and legs, and the blood pressure was thrown up to 170 m.m. *during the struggling*.

There is no question that the risk to the patient is increased by any struggling or tendency to struggle during induction. This was well shown in some cardiographs taken during some chloroform inductions that I had a few months ago. It is therefore all the more necessary that an anæsthetist should so perfect himself in the art of induction that

his patients do not struggle. To lessen this tendency for movement no point, no matter how trivial apparently, can safely be neglected.

A patient should always be induced in a position that is absolutely comfortable, and the anæsthetist should go to some pains to find out from the patient in which position he is most comfortable. A patient's hands should never be folded on his chest during induction, nor should the nurse grasp the patient's hands; but the patient may clasp the nurse's. The eyes should always be well covered during induction, and the patient should be informed why this is being done, in order to assist him to drift off to sleep the more easily and, also, to prevent any anæsthetic fluid or vapour reaching the eyes, because if it does it will make the eyes smart.

The most critical time in any induction is when the patient is passing over from the subconscious stage into the unconscious one. It is at that moment that he is most likely to move, and that likelihood to move is more certain to occur if the patient is not quite comfortable than if he is absolutely so and able to completely relax himself during the period of induction. The movement throws up the blood pressure and increases the heart's action, so that, in a patient whose heart is below par, it is quite possible that a fatality may occur, not necessarily at the moment when the extra strain occurs, but possibly later on during the course of the operation. This movement during induction also tightens up the muscles, so that a deeper stage of surgical anæsthesia is required to get complete muscular relaxation than in the case of a patient who has been able to pass over into his surgical anæsthetic sleep without any movement at all. In fact, it is remarkable in some patients with what a very small amount of anæsthetic you can obtain quite normal respirations, a good even steady pulse, with no sign of drop in blood pressure, and excellent abdominal relaxation without any straining: but, as I said before, to do so the eyes must be completely shut off from all light reflex. I consider the small preliminary of morphia and atropine, or some other premedication, an essential. There is no increased shock to the patient under these light anæsthetic conditions, but rather the reverse, in that anæsthetic shock is not added to surgical shock by overdosage.

As demonstrating the marked effect that holding the nose, whilst the mouth is kept shut, has on the rise in blood pressure, increase of pulse-rate, and increase in the depth of respiratory movements, let me draw your attention to some experiments carried out whilst I was under chloroform anæsthesia. One-sixth gr. morphia and 1/120th gr. atropine were given an hour before induction commenced. The administration was commenced sitting comfortably in an armchair. Just prior to commencing induction my blood pressure was 152 systolic, 80 diostolic, pulse 80, good, even; forty-six minutes after induction had been commenced, and on completion of the chloroform administration, the blood pressure was 130 m.m. systolic and pulse 66. Holding the nose and mouth closed, during the first half minute the blood pressure rose to 140 m.m. systolic, pulse 69; in the second half minute to 150 m.m. systolic, pulse 75; third half minute to 160 m.m. systolic, pulse 76; and breathing became deep and full. This administration of chloroform, nearly three-quarters of an hour altogether, was given throughout with the patient reclining comfortably in an easy chair.

On another occasion my blood pressure was taken prior to induction with nitrous oxide and after a preliminary of 1/8th gr. morphia and 1/150th gr. atropine, with the following results:— Blood pressure prior to induction 150 m.m. systolic; holding breath for a quarter of a minute, it rose to 160 m.m. systolic; holding the breath for half a minute, it rose to 172 m.m. systolic, with depened respirations.

It is always advisable towards the end of an operation, and before the patient's removal from the operating theatre, to deepen the patient's respirations. This can easily be done without the assistance of any cylinder of CO_2 (which we see so much advocated by some enthusiasts to-day) by the simple means of placing your hand over the patient's face and nostrils, so that the palm of the hand covers his mouth, and the thumb and forefinger gently close or partially close his nostrils. This obstruction to the freedom of respiration will within a few seconds deepen the respiratory movements considerably, and assist greatly in driving the anæsthetic vapour from the lungs, and help to rouse the patient.

It is very rarely indeed that endotracheal anæsthesia is

warranted, and the surgeon or anæsthetist who uses it unnecessarily is not playing cricket by his patient. There is hardly an instance in which either pharyngeal or naso-pharyngeal methods cannot be used equally well, or, indeed, with advantage as far as the patient is concerned as compared with endotracheal administration. As for the surgeon who desires endotracheal anæsthesia for abdominal work, I cannot speak too strongly in condemnation of such a man.

[Dr. Hornabrook here quoted support of this opinion from a surgeon and from a laryngologist.]

We come, next, to the tendency amongst some surgeons of too much use of local anæsthesia, especially for abdominal work or operations in the neighbourhood of the throat, without giving the patients the blessing at least of oblivion during such operations. I cannot too strongly stress this point. The nerve storm, or nerve stress, call it what you will, that a patient undergoes, especially in connection with any major operation, is, I regret to say, not always understood or grasped either by the surgeon or the patient himself at the time of operation. I have known cases operated upon under local anæsthesia, by some of the most prominent surgeons of our day, in which the patient has dreamt for months afterwards, in some cases for years, of the awful stress that he, quite unknown to himself, or unable to realize at the time, went through when he was operated upon under local anæsthesia and retained full consciousness. I have seen those patients placed in the dental chair, months afterwards, and be complete nerve wrecks if anything was attempted of even a minor nature in the way of touching up their teeth.

Upon one occasion four adult patients were down on the same morning for dissection of tonsils. Each had a preliminary 1/8th gr. morphia and 1/150th gr. atropine three-quarters of an hour before operation. In the first three cases, two adult females and one adult male, the pharyngeal method of ether administration by means of the Davis gag was adopted and a perfect anæsthesia, with retention of the swallowing and coughing reflexes, obtained for throat work, with no gagging when the tonsils were being dissected out. None of these patients gave the slightest trouble.

In the fourth case, an adult male, the surgeon thought he would like to use local anæsthesia, and this was done. The patient came away from the operating theatre a nervous wreck and showing marked signs of shock, with cold clammy skin.

On the following morning, when visited in the wards, all the patients who had been operated upon under general anæsthesia were in excellent condition, whilst the local man was still very down; he had asked the sister in charge why he could not have been operated upon under a decent method, like the others, and not tortured.

I am a strong believer in not bringing my patients into the operating theartre in a starving condition. Every one of my cases has two large slices of bread and butter with plenty of sugar on the bread two or three hours before operation, and is also always encouraged to have something such as water sago and sugar within a few hours following operation. Acidosis is not present immediately following an operation; but is encouraged by persistent vomiting upon an empty stomach.

The foregoing observations are the result of years of study and close practical personal contact with the various means and drugs employed in anæsthetic administration. According to all the theorists on anæsthesia, my organs should be a mass of degenerative cells. I am not aware, however, of any marked defect or abnormality in my kidney and liver secretions. I am not aware that my mental faculties have shown any marked falling off. My eyesight is certainly not as good as it was a few years ago, but at 60 years of age this is no more than to be expected.

[The address concluded with details of some cases showing the small amounts of anæsthetic needed to produce true "anæsthetic sleep."]

WHAT IS AERIS?

By Bellamy Gardner.

Late Anæsthetist Charing Cross Hospital, etc.

AERIS is the new name which I proposed for "laughing gas" and its modifications in a letter first published in the *Lancet*, for May 23rd, 1931.

It appears to me of primary importance to a patient requiring an anæsthetic that he should not be, as he often is at present, under a complete misapprehension as to the nature of the inhalation which he will be expected to breathe.

Owing to the prevalent use of the word "Gas" for nitrous oxide, many quite sensible people think it is a form of coal-gas, and are proportionately afraid of the ordeal.

Fortunately the words *anæsthetic*, *ether*, and *chloroform* convey no formidable nor unpleasant suggestion as to their qualities; but of late years the frequency of suicide by means of coal-gas, the terrible sufferings produced by chlorine compounds and mustard gas during the Great War—besides the familiar bad smell of an escape of gas from the household supply—have given the word gas a repulsive instead of an indifferent meaning.

It is for the purpose of rendering the beneficent and really somewhat delicious nitrous oxide attractive and soothing to those who will need it that I now press the medical and dental professions to adopt the new name.

Aeris is the genetive of the latin aër, meaning air, and, to my ears, is of a delightful, perhaps even effervescent, sound suggesting aeration and refreshment.

Scientifically speaking only the elements nitrogen and oxygen are contained in it: those which compose the atmosphere. What then more appropriate as a specific term for nitrous oxide than *Aeris*?

Though I fear that it may take 50 years to disseminate this new name, it will take longer still unless it is started now.

TWO CASES OF PULMONARY COLLAPSE.

By H. A. Richards, M.A., M.R.C.S., etc.
Anæsthetist to King's College Hospital, London.

A GIRL, aged 23, of poor physique and under-nourished, and the subject of mitral regurgitation, underwent the operation for radical mastoid cure. Induction was by gas and ether with a Clover, and gave no trouble. Maintenance of anæsthesia was carried out with open ether, which was soon changed for nitrous oxide given with CO_2 and oxygen, through a tube under a facepiece. Respiration was shallow and regular and the pulse of poor quality. After the operation had been in progress about ten minutes the breathing began to fail. There was no response to increased admission of CO_2 and the breathing ceased.

Artificial respiration did not revive it, and the post-mortem examination showed both lungs to be bright pink and completely collapsed, the appearance rather resembling that found after poisoning by carbon monoxide. The gas cylinders used in her case were, however, used again the same afternoon with ordinary effect, so that it is unlikely there was any carbon monoxide in the gas given to the patient who died.

The second case was that of a woman, aged 57, very fat and flabby, who was submitted to panhysterectomy for carcinoma of the body of the uterus. Stovain Billon (0.75 c.c.) was injected between the second and third lumbar spines and produced excellent anæsthesia. Unconsciousness was kept up with gas and oxygen and a little ether.

A steep Trendelenburg position was used. After about half an hour, the uterus having been removed, the breathing gradually failed. All the usual restorative measures were applied but were of no avail, and the patient died. Post-mortem showed complete collapse of the bases of both lungs. The heart was fatty and the muscle of the diaphragm fatty and inefficient.

ABSTRACTS.

Spinal analgesia in obstetrics. M. NANDESCAL. *Bull. et Mem. de la Soc. des Chirurg. de Paris,* 1931, xxvi, p. 140.

The author considers spinal the best form of anæsthesia in obstetrics except when version is required. The fact that uterine contractions are not diminished while paralysis of the cervix and of the perineum is complete gives spinal the superiority over other forms of anæsthesia in his opinion. He prefers it also for Cæsarean section. He uses the method especially when it is necessary to terminate labour although the os is not yet fully dilated, the artificial dilatation being rendered easy by the spinal injection.

A fatal anæsthetic explosion. Canadian Med. Assoc. Journ., April, 1931.

The accident occurred in an American hospital to a woman of forty-three who was inhaling nitrous oxide, oxygen and ether for the performance of a gall-bladder operation. The operation was about half completed when there was a slight explosion about the face of the patient. A slight burn was caused on the chin and the patient died immediately. At the autopsy the lungs were found badly torn. No damage was done to the equipment except to the rubber mask. Everything in use was found to be well grounded—operating table, gas machine, etc. A diathermy machine with a pad on each side of the patient was being used at the time of the operation "for supportive measures." This machine was in the hall outside, and the wires connecting it with the operating table were insulated. "There is no possible way of accounting for the static that caused the explosion."

The use of avertin in tetanus. New England Journ. Med., April 9, 1931.

Out of eleven cases of tetanus treated in Germany, seven recovered after the use of avertin. M. W. Huntington reports a case from the Yale University School of Medicine. The patient, aged eleven years, was seen in a typical spasm. Avertin was given with immediate relaxation of the spasm. This enabled antitoxin to be given, both intraspinally and intravenously. Avertin was given twice a day for ten days at the rate of 80 mg. per kilo of body weight and no ill-effects were noticed on blood-pressure or respiration. There was no habit-forming from the drug, and complete recovery was obtained.

In the same issue of the journal is an article describing the use of luminal and amytal in thyroid surgery. Luminal is given in increasing doses for several days before operation. The day before operation one grain is given morning and noon and one and a half grains at night. Two hours before operation two three-grain capsules of amytal are given by the mouth and one more capsule one hour before operation. The patient is very drowsy by the time for operation. Gas and oxygen are given directly she is on the table. Fourteen consecutive cases treated by this method have been most satisfactory.

The alkaline reserve of the blood in experimental narcosis with ethylene and with ether. G. NOGARA. *Annal. Ital. di Chirurg.*, April, 1931, pp. 298 *et seq.*

The experiments were conducted for ten days, the blood being obtained from the femoral artery. The result of the author's experiments and calculations was that he showed that the curve representing the alkaline reserve of the blood was modified to a much less degree after one hour's narcosis with ethylene than it was after the same time with ether. Moreover—and this he regards as more important—the normal was reached again much more quickly after ethylene that after ether. A good summary of previous experimental work with ethylene precedes the account of his work.

Spinal anæsthesia. Schmerz Narkose-Anæsthesia, April, 1931.

Both v. Jaschke and A. Mayer contribute articles in praise of the routine use of spinal methods in gynæcology. Jaschke uses tropocaine and says that his further experience during the last nine years convinces him that spinal injection with this drug is the best and safest method of anæsthesia for gynæcological work. Failures and dangers he attributes always to faulty technique. For subsequent headache, which he places at 3 per cent of the cases he uses intravenous infusion of 1000 c.c. saline. Mayer uses novocaine and he expects headache in about 12 per cent of his patients. The causation he regards as quite unexplained and the treatment was unsatisfactory.

Pharmacology of local anæsthetics. H. W. COLES and HELEN ROSE. *Anæsthesia and Analgesia*, May, 1931.

The authors carried out an experimental study on dogs with neothesin. This is "the best and most practical local anæsthetic synthesized by McElvain and co-workers" and is structurally allied to cocain and procain. The conclusions arrived at are mainly that when given in convulsive doses (75 mgr. per kilo of animal's weight) most of the anæsthetic is eliminated within eight hours. The dog is able to break down neothesin with basic substances titratable with hydrochloric acid. The ether extract residues of the urine of dogs

injected subcutaneously with large amounts of neothesin produce strong and lasting anæsthesia in the rabbits cornea. Little unaltered neothesin is secreted in the urine of the dog. Neothesin is relatively non-toxic and is easily detoxicated and eliminated.

Anæsthesia in diabetics. T. T. LEHMANN. *Anæsthesia and Analgesia,* May, 1931.

Observations were made on seventeen diabetics to whom a general anæsthetic was administered for operation. Complete records were obtained of pre-operative and post-operative blood sugar and plasma CO_2 determinations. The blood was taken just before operation and at various times after its completion. The patients were prepared by appropriate diet and insulin. The anæsthetic was usually ethylene with gas and ether in one case and gas and oxygen in one case. There was in no case any approach to acidosis. The method included an extra carbohydrate feed, with insulin, six to eight hours before operation, and 20 c.c. of 50 per cent glucose intravenously during operation with one to two units of insulin per gram of glucose.

The effect of opening the peritoneal cavity. M. H. OVERHOLT. *Anæsthesia and Analgesia,* May, 1931.

In explaining the depressing effects on respiration of an abdominal operation the author points to the alteration in pressure within the peritoneal cavity and consequent alteration in position of the diaphragm. Intra-peritoneal pressure in the immediate subdiaphragmatic region is normally below that of the atmosphere. Opening the abdominal cavity alters this. An atmospheric pressure is exerted below the diaphragm and permits the subatmospheric intra-pleural pressure to pull the diaphragm to a higher position within the thorax.

When the abdomen is opened the effect of the weight of the viscera and the resistance of the anterior abdominal wall are abolished and the diaphragm must alone resist the negative intra-pleural pressure. Excursions of the diaphragm are reduced after the abdomen has been opened. A mechanical disadvantage is imposed on its activity because of its abnormally high position. The alteration in position and activity of the diaphragm consequent on opening the abdomen have been shown experimentally with X-rays as well as clinically. There is thus decrease in lung volume as well as a comparative stagnation of air currents. These factors play an important part in post-operative pulmonary troubles. The article concludes with a number of recommendations for improving post-operative pulmonary ventilation.

Anæsthetic explosions. Journ. Amer. Med. Assoc., May, 10, 1930.

The report of the American committee on accidents in anæsthetics provides a full consideration of the frequency and causation of explosions. It states that probably there are more explosions from ether than from any other agent, but these are generally trivial whereas those from ethylene are usually grave. Explosions due to electrostatic discharge and those due to defectively wired head-lights, to sparks in motor and suction apparatus and to diathermy and radiology instruments are among the kinds discussed, and a number of rules are given for the avoidance of these accidents. It is mentioned that the mixtures of ethylene and oxygen generally used for analgesia in labour are highly explosive, the most explosive being the mixture of 25 per cent ethylene with 75 per cent oxygen. The mixtures stronger in ethylene, more commonly used in surgery, are less easily fired.

The committee are of opinion that the advantages of modern methods of administration and of the machinery involved more than counterbalance the disadvantage of possibilities of explosion. The hazards of the latter are far less, they believe, than the hazards of post-operative pulmonary trouble involved in the older method of inhalation anæsthesia.

Damage to the bowel after avertin narcosis. Professor I. WYMER. *Schmerz, Narkose-Anæsthesia*, April, 1991.

The author reports the case of a man of 60 undergoing an operation for stone in the bladder. The dosage of avertin administered was unusually high (0.16 gr. per kilo) and unconsciousness came on rapidly after the injection. Mucus and blood were discharged per rectum early in the recovery period, and the man died on the third day. Post-mortem extensive ulceration of the colon was discovered. There was also a small bladder perforation apparently in connection with the operation and some early peritonitis in this neighbourhood. It is remarkable that the bowel was quite free from damage so far as the rectum, sigmoid and descending colon were concerned. All the trouble was in the ascending colon. The suggestion was made that the injection had perhaps been too fiercely carried out. The author carried out some experimental observations with two per cent solution of avertin injected subcutaneously in guinea-pigs, and points out that after eight to ten days there was necrosis of the skin. Control injections of saline produced no corresponding phenomenon.

Post-operative pulmonary atelectasis. A. L. BROWN. *Archives of Surgery*, June, 1931.

The author maintains that bronchial obstruction plus decreased aeration ability of the lungs is a prime cause of post-

operative pulmonary atelectasis and that the obstruction is usually due to retained tracheo-bronchial secretions. Limitation of movement of the diaphragm greatly aids the process. He thinks that the part of the bronchial secretions producing the atelectasis or determining its type has not been given sufficient consideration. Whereas the usual action of cough is to expel material from the tracheo-bronchial tree it may, under certain conditions, bring about the opposite result, and drive the material deeper down. The degree of penetration of the material into the smaller ramifications of the tracheo-bronchial tree is in direct proportion to the viscosity of the material. The less viscid it is, the greater is the penetration and the possibility of dispersion. The usual type of secretion is a thick viscid material which plugs a bronchial orifice, atelectasis occurring distal to the plug. If the material is slightly less viscid one or more smaller bronchii may be plugged and so on, till one may have, with a watery secretion, the condition known as "drowned lung." The authors experiments with intra-tracheal injections and X-rays appear to support his view, which is well illustrated by a case quoted.

He holds also that pulmonary atelectasis follows spinal anæsthesia more frequently than it does any form of inhalation or regional anæsthetic, regardless of the type of operation. This he attributes to the fact that "spinal anæsthesia definitely inhibits the depth and force of respiratory movements, not only during the operation itself but for a considerable period after."

In considering the effect of this form of anæsthesia on arterial tone Burch and Harrison (*ibid*) state that the constrictor response to hæmorrhage is abolished by spinal anæsthesia, and that this probably explains the unusual susceptibility to hæmorrhage of patients and animals under its influence. With regard to the fall in blood-pressure, they believe that on experimental grounds the administration of salt solution intravenously immediately before the induction of spinal anæsthesia should be valuable.

Hyoscine anæsthesia in labour. Can. Med. Ass. Journ., June, 1931.

Dr. A. Somerville draws attention to the merits of hyoscine hydrobromide used *alone*. She gives 1/100th grain of hyoscine hydrobromide at half-hour intervals for three doses and then every two hours. In primiparæ the administration is started when pains are at five minutes intervals, in multiparæ when labour is definitely established. As much as 1/25th grain has been given harmlessly in an hour. The pains were increased in strength and frequency by the hyoscine. The treatment requires the presence of an obstetrician throughout from an early stage in the labour, but this is definitely shortened in duration.

REVIEWS.

Practical anæsthetics. CHARLES F. HADFIELD, M.A., M.D. (Cantab.), Practical Handbooks Series, Ballière, Tindall and Cox.

Dr. Hadfield's excellent book of eight years ago here makes its reappearance—this time in the "Practical Handbooks Series." As a general but simple discussion of anæsthetics and the practical problems of their administration it has not been, and is not likely to be, improved upon. Recent advances and changes in methods have necessitated some revisions and certain extensions, but the original aim of clearly expounding the general phenomena and basic methods of anæsthetics is still most adequately achieved. Dr. Hadfield, rightly, we consider, fights against the increasing mechanisation of anæsthetic administration—not as an anæsthetist, but as a teacher. He maintains in all rightness that the "rag and bottle" methods are the only ground-work upon which a sound knowledge of anæsthetics can be built.

In view of this standpoint we are surprised that the description of the Vernon-Harcourt inhaler is still included. The increasing use and demand for "gas-ogygen-ether" anæsthesia well justifies the full description of the Boyle apparatus. As nearly every cottage hospital now has such a machine it may well be regarded as in common use and within the range of the non-specialist anæsthetist. We are glad, too, that Dr. Hadfield insists upon the essential part played by ether in this method, even though the quantities used are remarkably small. Many a young anæsthetist has nearly broken his own and the surgeons' hearts by endeavouring to keep to pure gas and oxygen.

It is also rather confusing to find that the C and E mixtures are treated with such faint praise as almost to damn them. As inducing agents we consider them extremely valuable: even the 1 in 16 and 1 in 32 concentrations have definite advantages over pure ether. In the description of nasal gas it is a pity that the McKesson type of nose-piece is not described and recommended. Its ready application to noses of every shape, its easy fixing to the head, its easy sterilization, and its freely controlled expiratory valve make it extremely useful and of great advantage—all due to its extreme simplicity.

These few criticisms apart, we cannot but give unstinting praise to this new edition of an excellent book. The additions in the way of describing avertin, percain, etc., are short, simple and balanced. The old matter has been modified and extended sufficiently to retain

and enhance its previous excellence, and there has been produced a first-class handbook on practical anæsthetics well deserving of the success which it will almost certainly attain. G. E.

An A.B.C. of Medical Hypnosis. By E. HOPEWELL-ASH, M.D. (Lond.). (Published privately).

This little book has been written "to provide colleagues with just as much practical knowledge of the elements of medical hypnosis as may be useful to them." We have no doubt that a working knowledge of the principles of hypnosis is a very useful asset to the practising anæsthetist and many may find in this small book material to fill gaps in their information. Dr. Ash regards "medical hypnotism" as a technical process for inducing demonstrable hypnosis for the purpose of increasing receptivity to suggestion, personal magnetism, auto-suggestion, spiritual healing and other terms including the psychotherapy of transferred therapeutic influence. In their practice many expert anæsthetists, we have no doubt, do increase the receptivity of their patients without actually inducing manifest hypnosis. They employ suggestion in the skilled handling of frightened or antagonistic people before and in the early stages of induction. Dr. Ash is of opinion that hypnotic analgesia would be extremely useful in minor surgery and in operations on the nose, throat or eyes, to say nothing of dental extractions.

There is not, however, any certain method of producing hypnotic anæsthesia rapidly, and several sittings may be needed to get a satisfactory result. The advantages of this form of anæsthesia are its safety, that it can be prolonged at the will of the operator, that there are no unpleasant after-effects, and in mouth operations the fact that the jaw can be fixed open without the use of a gag. Reflex movements can be entirely avoided by giving the requisite suggestions.

The little book contains a very clear exposition of exactly what constitutes the hypnotic state, this being of interest and importance to anæsthetists as to all other medical men.

J. B.

CORRESPONDENCE.

31, Billing Road,

Northampton.

3rd April, 1931.

To the Editor of the *British Journal of Anæsthesia*.

Dear Sir,

As an addition to Dr. Moir's letter may I acquaint you with the arrangements which I have made at the Northampton General Hospital?

I am the only Honorary Anæsthetist and receive no honorarium or payment of any kind. I am a member of the medical board and board of governors. I attend the hospital five mornings and four afternoons a week, taking off what time I need for private work, the surgeons always willingly altering their lists to release me for private work while still retaining me for difficult hospital cases. It is also generally understood that for any dangerous or difficult urgency I should, at least, be consulted, and in the last eighteen months I have never failed to answer a hospital call. For this, again, I receive no payment. Urgency work comes most heavily upon me when we have a change of house surgeons as the honorary staff have to get to know their capabilities, but I have so far managed to cope with the work. The consideration I receive from the surgical staff is such that a really heavy appointment is workable, and sufficient to see that I am not called to hospital except for bona fide reasons.

I am, yours etc.,

F. F. WADDY.

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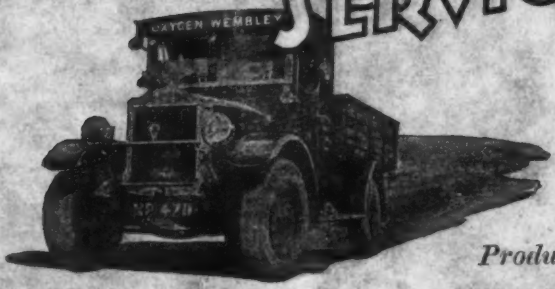

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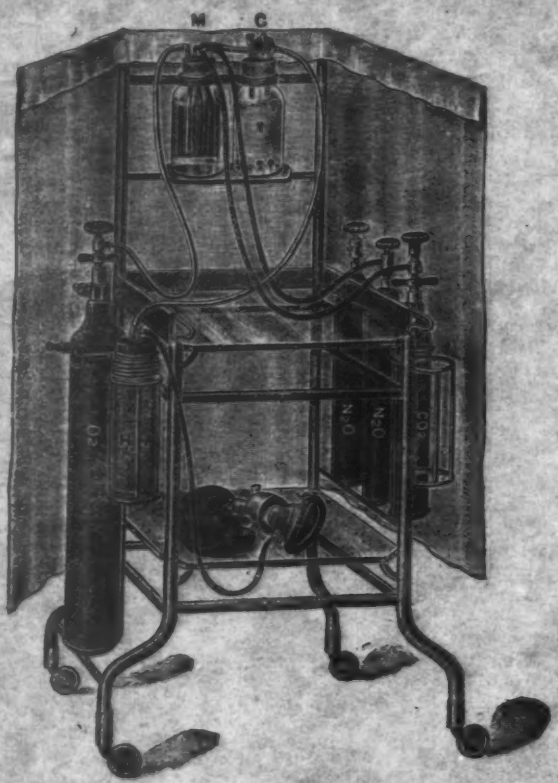

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